Peer-review report of

Miklikowska, M. & Besta, T. (2026). System-disillusioned youth radicalize: The role of political alienation in the formation of political radicalism. *advances.in/psychology*, 1, e716424. https://doi.org/10.56296/aip00061

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Round 1

Editor

Date: 2026-03-23

Dear Authors,

Thank you for submitting your manuscript, “System-Disillusioned Youth Radicalize: The Role of Political Alienation in the Formation of Political Radicalism,” for consideration in our special issue, *Advances in the Psychology of Violent Extremism*. I have now received feedback from two experts with highly relevant knowledge in this area. I am grateful for their thorough reviews. As is standard for this journal, I assessed the manuscript independently before reading the evaluations.

Both reviewers highlight several strengths of the manuscript. Reviewer 1 notes that “there is much to appreciate about this manuscript,” emphasizing in particular that “the data are impressive if not unique” and that the study addresses a clear gap in the literature. Similarly, Reviewer 2 points out that “the longitudinal data and the differentiation between processes are strengths of the paper,” and further highlights that the manuscript is “well written and relatively easy to read.” I agree with these assessments and see clear potential in the contribution.

At the same time, both the reviewers’ comments and my independent assessment converge on several substantive concerns that need to be addressed before the manuscript can be considered for publication. I would therefore like to invite you to revise your manuscript in light of both the reviewers’ feedback and my own assessment. Rather than reiterating all points in detail here, I encourage you to engage carefully with the full reviews below. In the following, I summarize and synthesize what I consider to be the most central issues.

First, there are important concerns regarding the conceptual framing of the study. In its current form, the manuscript relies heavily on the Significance Quest Theory as its primary framework. However, Reviewer 2 —and I concur—raises doubts about whether this framework is the most appropriate for capturing the processes under investigation, particularly given that the key constructs (e.g., political alienation, distrust, dissatisfaction) appear more directly aligned with grievance-based or political alienation perspectives.

Second, and most critically, there are substantial concerns regarding the operationalization of political radicalism. As noted by Reviewer 2 and echoed in my own reading, the behavioral indicators used do not unambiguously capture radicalism. Several items may reflect normative or situational forms of political participation rather than ideological extremity, and the distribution of the variable suggests that the vast majority of respondents report no engagement in such behaviors.

This raises serious questions about construct validity, interpretability, and the extent to which the study can meaningfully speak to radicalization processes. At minimum, this issue needs to be explicitly acknowledged and discussed as a key limitation, and the interpretation of the findings should be substantially tempered.

Third, a number of methodological issues require attention. These include the need to test and report measurement invariance across time, provide a clear account of attrition and missing data handling, and consider models that allow for stronger claims about temporal ordering (e.g., lagged or cross-lagged approaches). In addition, the skewed distribution of the outcome variable raises concerns about model assumptions, which should be addressed through appropriate robustness checks.

Fourth, the manuscript would benefit from greater conceptual and analytical precision in distinguishing between political participation, non-normative behavior, and radicalism. At present, these constructs appear to be used somewhat interchangeably, which risks overstating the conclusions that can be drawn from the data.

Fifth, several issues related to sample characterization and interpretation should be addressed. In particular, the operationalization of immigrant background is quite broad and collapses meaningful heterogeneity. This category likely includes individuals with substantially different developmental and socialization trajectories (e.g., first-generation vs. second-generation, age at migration), which may have important implications for the outcomes under study. This should be acknowledged more explicitly, and interpretations should be correspondingly cautious. Prior research has documented meaningful differences between native- and foreign-born individuals in their endorsement of violence and extremist attitudes (e.g., my own work, Obaidi et al., 2019), further underscoring the importance of not treating this group as homogeneous.

In addition to these larger overarching points, please address the following specific issues in your revision.

Stylistic Issues

· There are a few stylistic and formatting details that require attention. Please present effect estimates for the *F*-tests, and also report the means and 95% confidence intervals for these comparisons. Some *F*-stats have three digits. Please change to two.

· Please make sure to italicize all Latin statistical letters, such as *M* and *SD*, and italicize all scale endpoint labels.

· When reporting statistics, present exact 3-digit *p*-values for all tests, including non-significant ones, replacing instances of “n.s.”.

· Please ensure you consistently report 95% confidence intervals for all tests, as they are currently missing for some statistics, for example on lines 380 and 382.

· Furthermore, please add a space between “95%” and “CI” throughout the manuscript.

· There is a typo in the item text that reads “Those in power makes decisions without finding out what people like me think” which should be corrected.

· There are some typos (e.g., wrong year) and other smaller issues in the citations and references. Please use our free tool at https://apa.advances.in to identify these. Please note that the tool is probabilistic so there will be false positives that can be ignored.

· Please also make sure that every journal article in the references has a doi number in the URL format that is required by APA7.

In sum, the manuscript addresses an important topic and has several notable strengths, but substantial revisions are necessary, particularly with respect to theoretical alignment and the validity of the key outcome measure, before it can be considered further.

I look forward to receiving a revised version that engages thoroughly with these points as well as the detailed reviewer comments below. Please submit your revision by May 23.

Best regards,

Milan Obaidi

Reviewer 1

This paper uses 5-year panel data for examining the development of radical political behavior among a sample of Swedish adolescents (13 to 17 years). The longitudinal data and the differentiation between processes are strengths of the paper. Involvement in radical political behavior is found – at the within-person and between-person level – to be associated with political distrust, powerlessness, and dissatisfaction. The paper is well written and relatively easy to read and appropriate statistical analyses seem to be conducted: yet, I am not an expert in this type of analyses and cannot evaluate all the analytical decisions. I have several comments that hopefully are of some use for the author(s) and Editor.

My strongest concern and reservation is about the radical political behavior assessment. The paper claims that it examines political radical behavior rather than radical attitudes. However, firstly, the focus is on *self-reported* behavior (which is only mentioned once, p. 22). Secondly and more importantly, on average no or hardly any radical behavior is reported. The measure for radical political behavior uses a 3-pont response scale in which a score of 1 means NO (not being involved in this type of behavior the last 12 months), and the score 2 = ‘once or twice’. Table 1 shows that, for example at T3 and T5 the mean score is 1.08 and the SD = 0.18, and at T3 the

mean is 1.03 (SD = 0.15). Thus on average the answers of this sample of youngsters is NO - not being involved in political radical behavior - and this can also explain the high internal reliabilities of the 5-item scale. This raises the central question of what exactly the paper and analysis is about: what do we learn from engagement in political radical behavior when no such behavior is reported by the majority. It seems like trying to understand why youth uses alcohol or drugs by studying a sample that is not involved in these risky behaviors. I think this is quite problematic or at least seriously limiting the contribution and value of the study. It probably also implies that the dependent self-report measure is quite skewed (it would be useful to give the percentages for the three response categories) and that an analysis using a dichotomous outcome (No vs. Yes) is more appropriate and this might give somewhat different results.

The average finding of 'No' self-reported radical political behavior might have to do with the specific period (data are some 15 years old), sample (school) and situational context (city), but also with the age group. For example, how meaningful is it to ask, say, a 13 or 14 year old kid about having "Taken part in an illegal action/demonstration or occupation", "Taken part in a political event where property was destroyed", or having "Broken the law on political grounds". Is there any independent evidence (e.g., official statistics, police records) that in a high trust society like Sweden 'normal' youth of this age tends to be involved in these kinds of behaviors? And, furthermore, do these questions – and also the political alienation questions that appear to have been taken from research among adult samples: is there any evidence that these questions are also okay for young adolescence? – have the same meaning for the different age groups? Furthermore, several of these questions seem to assume a level of political knowledge (e.g., of the political system) and political interest. Perhaps the original dataset contains measures of political knowledge and political interest which would be useful to consider, for example as relevant control variables or moderators.

In how far is radical political behavior for 13-15 year olds a means of restoring a diminished sense of significance due to negative political, social, or personal experiences. In other words, is the Significance Quest Theory the most appropriate framework for understanding the self-reported behavior of 'common' Swedish youngsters? The empirical evidence in support of this framework that is discussed on pages 4-5 is all about adult samples. The literature on political radicalization uses various other theoretical approaches that might be of use, but these are all ignored.

On p. 7 it is stated that "We further expected that adolescents with *higher* overall levels of political alienation would exhibit *lower* overall levels of radicalism…". I do not fully understand the basis of this prediction and how it is derived from Significant Quest Theory. Or is it a typo?

The findings mainly apply for boys rather than girls but almost nothing about possible gender differences is said in the Introduction. Furthermore, the findings are mostly discussed in terms of adolescence in general rather than male adolescence in particular.

Reviewer 2

In their manuscript, the authors present an analysis of a longitudinal dataset, demonstrating that within-person changes in three measures of political alienation are positively correlated with within-person changes in radicalism. There is much to appreciate about this manuscript. It addresses a clear gap in the literature, as developmental perspectives on radicalization and violent extremism

have been theorized but remain empirically relatively unexplored. The data are impressive if not unique, and the analyses are rigorous, although I note several avenues for improvement below. The paper also aligns well with the scope of the special issue. Therefore, I would be glad to see this paper published, provided some substantial revisions are made.

Major points:

Although I applaud the longitudinal analyses, several methodological issues require attention. First, given the large time span, which is a clear strength of the study, the meanings of constructs can change. Formal tests of measurement invariance over time are therefore necessary and should be provided in the manuscript.

Second, the absence of reported attrition rates is surprising. Formal tests are needed to determine whether attrition was selective based on the core study variables and demographics, along with a clear explanation of how missing data were handled. Given that Mplus was used, I assume full information maximum likelihood was employed, but this must be explicitly stated, and evidence that the missing at random assumption is met should be provided.

Third, considering the impressive dataset, it is unclear why no model was estimated to test for the time precedence of the variables. Based on the introduction, I anticipated a random intercept cross-lagged panel model, following Hamaker and colleagues. Such a model would be highly beneficial for determining whether a change in one variable precedes a change in another. If this approach is too complex given the multilevel structure, although Mplus can typically handle this, I assume the estimated random intercept multilevel model could at least be lagged to explore potential time-precedence relationships.

Fourth, the radicalization variable is extremely skewed and exhibits very little variation. The authors should address whether this threatens the model assumptions. It would be useful to know if testing a binary coding or another type of transformation would alter the results. Exploring alternative methods, such as bootstrapping, could help ensure the findings are robust to this non-normality.

Fifth, the visualizations are not entirely effective. For example, the rationale for choosing minus two and plus two standard deviations for the visualization is unclear. Overall, the graphs are not very intuitive. I would prefer Neyman-Johnson floodlight graphs, where time remains on the x-axis, but the y-axis represents the association of each of the three variables with radicalism.

Sixth, while the sample size appears adequate, presenting a power sensitivity analysis would be beneficial. I understand that the tested model is complex, but providing an approximation would be helpful, especially since the class level seems to have a relatively low number of units.

Regarding the introduction, it is well written and builds a relatively strong case for the study objectives. However, two aspects warrant further attention. First, it is well established that almost all violent extremism is perpetrated by men, and there is evidence of selective radicalization among youth, including research published in this journal (https://doi.org/10.56296/aip00042). I am pleased that the authors tested for gender differences, but the introduction should better prepare the reader for these analyses. Furthermore, these gender findings are highly relevant for policy and should be highlighted in the abstract. Additionally, while the investigation of gender differences is commendable, a visualization of this interaction would be important.

Second, the theoretical foundation for the three subconstructs used to measure political alienation feels somewhat underdeveloped. The authors should clarify why these specific subconstructs are central to the overall construct and important for the project. As they likely know, several of them (e.g., distrust) has many of their own theoretical frameworks. It is also unclear whether an individual is considered alienated if they score high on just one subconstruct or if high scores on all of them are required. Deeper conceptualization and theorizing in this area would greatly strengthen the manuscript.

Finally, while the authors generally describe their correlational data adequately, the phrasing occasionally implies causality. For instance, the authors state that alienation changes "predict" radicalism, but the relationship could be the opposite or bidirectional, and the data only demonstrate an association. Parts of the discussion also adopt a causal tone, such as mentioning the "role of political alienation … in the formation of radical political activism." The word "formation" lends a causal implication to this sentence. There are several other instances of this, such as the claim that the variables "translate into radical political behavior." This phrasing overstates what the data can empirically support.

More minor issues:

In the limitations section, the authors write, "However, controlling for key background factors did not alter the results substantially…" This statement does not appear entirely accurate, as several effects became non-significant. It is also somewhat contradictory to discuss the possibility of controlling for mental health while simultaneously arguing that it is not highly predictive of extremism.

The paragraph beginning with "The present study advances research on political radicalization…" would likely be better positioned under the heading "Present Study."

Regarding the hypothesis on line 165, it is unclear if there is a typographical error. Did the authors mean that individuals "with higher overall levels of political alienation would exhibit HIGHER overall levels of radicalism"? If not, this requires further explanation, as it appears contradictory to the within-person prediction.

The operationalization of immigrant background is problematic. If both parents are from Finland, it is unclear why this would not be considered an immigrant background, as it would be by international standards. Recoding the data to reflect this would be ideal. If recoding is not feasible, this must be acknowledged as a limitation, and the variable should be renamed to something like "non-Nordic immigrant background".

The authors test both quadratic and linear slopes, but the rationale for including these should be provided in the introduction.

In the figures, the ribbon color shading obscures the lines. Making the shading more transparent would improve readability. Additionally, the authors should clarify why the predictions are presented with 90% confidence intervals rather than the standard 95% intervals.

Since Mplus can provide standardized β coefficients, these should be included alongside the unstandardized *B* coefficients. Without both, interpreting the strength of the observed effects is quite difficult.

On line 411, the text suddenly refers to ethnicity instead of immigrant background, which should be corrected for consistency throughout the manuscript.

There is an inconsistency in terminology: in one instance, the text refers to “proscriptively prosocial institutions” and in another, “prescriptively prosocial institutions.” In both cases, the intended meaning is somewhat ambiguous and requires clarification.

Conclusion

In summary, I enjoyed reading the manuscript and found the research to be generally well executed. I hope these comments assist the authors in further strengthening their work.

Round 2

Editor

Date: 2026-05-04

Dear Authors,

Thank you for your revisions. The manuscript has clearly improved, and both the structure and clarity of the analyses are much stronger. I appreciate the effort you have invested in responding to the previous round of comments. One reviewer notes additional important issues, including some that were not addressed fully in the previous round. Please make sure to address each of them carefully in the next, and hopefully final, version.

I am in general agreement with the Reviewer’s comments. For instance, I agree that you should add the one RI-CLPM to the main text. Please ensure it is accompanied by a figure displaying exact *p*-values alongside 95% confidence intervals. Additionally, please report the previously requested figure illustrating gender differences. This represents a key finding of typical interest to readers.

In addition, please resolve all inconsistency between the presentation of uncertainty in the text and in figures (including the additional gender plots). As the Reviewer suggested, please change the confidence bands to 95% CI. Also make sure to use standard +/-1 *SD* for visualization. You can use transparent bands to ensure visibility.

Please also address all remaining points noted by the reviewer.

While reading the revised version, I noted a small number of additional points that I would encourage you to review carefully before the manuscript can be finalized. These are mostly issues of internal consistency, clarity, and precision rather than major conceptual concerns.

In the dissatisfaction model (page 9), there appears to be a potential inconsistency in the reported estimates. For example, a coefficient is reported as *B* = 0.50 with a 95% confidence interval of [0.017–0.083]. It seems unlikely that these values correspond to the same estimate. Could this be a typographical error (e.g., *B* = 0.05)? I request that you carefully verify this and similar instances to ensure consistency between coefficients and confidence intervals. This is critical statistical information and there is no room for error.

My initial instructions regarding the italicization of scale point labels appear to have been misunderstood. In accordance with APA 7 guidelines, the text labels (e.g., *A lot of trust*) should be italicized, whereas the corresponding numbers (e.g., 1 =) should not.

Despite previous requests, Latin statistical symbols are still not consistently italicized throughout the manuscript (e.g., “N”, or “B” on line 416, among several other locations).

Furthermore, there are remaining instances where cutoffs are reported instead of exact, three-digit *p*-values (e.g., “*p* > .05”). Please solve these issues.

On line 443, certain estimates are still reported to three decimal places. In the previous round, a two-decimal format was requested (except for *p*-values, and you confirmed this change had been implemented).

As noted previously, the space between “95%” and “CI” is still missing in several cases. Please also ensure that upper and lower bounds are delineated with a comma rather than a “minus” sign (for example, the formatting “-0.048-0.058” is confusing). Several estimates (e.g., on line 391, among other instances) still lack 95% confidence intervals entirely. Finally, in terms of order of information, please verify that confidence intervals consistently follow the relevant estimate rather than the *p*-value (you sometimes present 95% CIs at the end after the *p*-value, which can introduce ambiguity concerning what estimate it is related to).

In Table 1, it appears that several effects denoted as ***p* < .01 are actually ****p* < .001. Please report three significance levels in it.

Linguistic Issues

Please change the term “insignificant” to “non-significant”.

There is a word missing in the following phrase: “which may individuals to challenge existing structures,”.

Typographical errors (e.g., “Eruropean”; “grand-mean cantered”) and missing articles (such as “a” or “an”) remain throughout the manuscript. A careful proofread is necessary.

On line 468, there is a missing word: “to dissatisfaction [missing “from”] T1-T2, T2-T3…”.

Regarding the phrase “The effects of distrust have not changed substantially when…”, please revise to “did not change” in this instance and consistently throughout the text.

I would also encourage a careful check of the supplementary materials. There are a few places where the text appears inconsistent or possibly carried over from earlier drafts (e.g., shifts in construct labels, duplicated sections, or isolated wording inconsistencies). For example, in the section “Political dissatisfaction”, there is a sentence referring to “At the between-person level, there was a significant correlation between peer harassment and radicalism (r = .15, p = .051).” While these do not affect the main conclusions, ensuring that the supplement is fully aligned with the main text is critical.

Relatedly, there may be another inconsistency in the following passage: “There was a bordeline significant within-time association at T1 (r = .025, p = .056) and bordeline significant correlated changes between powerlessness and radicalism (r = .029, p = .055; r = .50, p = .054, r = .058, p = .055, and r = .050, p = .054, at T2–T5 respectively)…”. All other correlations in the same section are around .02–.06, whereas “.50” is substantially larger. It is unclear whether this reflects a typographical issue (e.g., .050) or a correctly reported value that may require clarification. Please carefully double-checking all stats for consistency so we don’t need another unnecessary round.

Citations and References
Unfortunately, substantial issues persist within the reference list. I have provided some examples below, but please carefully verify all references using our free tool at https://apa.advances.in. Typically, when a reference is flagged by the tool, it indicates an error, which may be as minor as a missing period after an author’s name, a typographical error, or a similar oversight. For instance:

1. The reference list shows “Ungdomsbarometern” instead of “Ugdomsbarometern”. Please reference the barometer directly rather than the media page, or alternatively, cite SVT. Furthermore, this reference is missing a title.
2. This reference leads to a report with a different name: “UN (2022). United Nations Report of the Secretary-General on Violent Extremism. https://digitallibrary.un.org/record/3957700?v=pdf”
3. This reference also lacks a title: “Statistiska Centralbyran (2016). Retrieved from: http://www.statistikdatabasen.scb.se/pxweb/en/”
4. Please make sure there are no missing titles for other references.

5. The following link does not direct to the specified report and requires updating: http://www.europarl.europa.eu/supporting-analyses
6. A comma is missing in the reference list between last name and first name initials, e.g. “Deželan T.” or “Rydgren J.”. These are other standard formatting errors that our tool will easily detect.
7. A period is missing in the reference list after “Souleimanov, E.A”. There are numerous instances where first names do not conform to APA 7 formatting.
8. This reference requires a comprehensive revision: “Van der Toorn van Stekelenburg, J., & Klandermans, P. G. (2013).”
9. Beelmann is entirely missing from the reference list, likely due to a misspelling of the name in the text.
10. Hamaker is also completely missing from the reference list.

Please note that these are merely illustrative examples. A thorough review of all entries is required. There will be no room for further oversights of this nature in the subsequent version of your manuscript.

In sum, I believe that the current version has made considerable progress and I thank you again for your attentiveness. At the same time, some additional work is needed to satisfy the quality threshold we have for this special issue. Therefore, I invite you to submit a revised version by June 4. Conditional on how thoroughly the issues raised in this letter and by the Reviewer are addressed, I plan to make a decision in the next round without consolidating further reviews.

Best,

Milan Obaidi

Reviewer 2

It was a pleasure to re-evaluate this revised manuscript. I remain positively inclined toward the work and believe the revision represents a significant improvement. The enhancements regarding conceptualization and the addition of robustness tests are particularly notable. At the same time, I was dissatisfied with how certain previous requests were addressed. Some of my current comments are presentational, while others are more substantive, highlighting areas where I disagree with the authors regarding their analytical choices and corresponding justifications. I believe this needs more consideration.

Use of the SOM

I agree that many of the supplemental analyses are highly valuable and appropriately placed in the SOM to maintain the focus of the manuscript. However, some of these results require a more thorough summary within the main text to proactively address potential reader concerns.

First, the measurement invariance and dropout tests should be briefly summarized in the main text rather than merely cited, as they are crucial for understanding the methodology and your sample. Additionally, I was somewhat surprised by the authors describing "R^2 = .067" as low in their dropout analysis. This level of explained variance is typical in psychology research and not particularly low. I invite the authors to briefly acknowledge this in the limitations section concerning data representativeness.

Second, I appreciate the inclusion of the random intercept cross-lagged panel models, which substantially increased the depth of insight into the data. I agree with how this information is presented in the main text, with one exception. For the final model, where several significant cross-lagged effects were observed, I strongly suggest moving these results to the main text and including a figure to illustrate the model. This finding elevates the paper by providing evidence of temporal precedence. Please also mention this finding in the abstract as such reciprocal feedback-loops will be of significant interest to the research community.

Third, thank you for including the robustness tests utilizing binary variables. These analyses increase the credibility of the findings and should also be briefly summarized in the main text with a reference to the SOM.

Remaining Unaddressed Issues

Contrary to my previous recommendation, the authors did not alter their figure presentation. While I am amenable to retaining the general format, several critical issues remain and must be addressed.

Regarding my initial comments on the figures, I appreciate the provided explanation, but the justification for these visualization choices is methodologically inappropriate. The authors state they reduced the confidence intervals from the standard 95% to 90% and chose to plot the extremes of +/- 2 standard deviations specifically to avoid overly wide and overlapping bands. Adjusting the uncertainty threshold and plotting extreme percentiles to artificially prevent error bands from overlapping constitutes visual manipulation that misrepresents the actual variance in the data.

As the authors are likely aware, it is standard practice in the social sciences to plot marginal effects using +/- 1 standard deviation to represent high and low groups, providing a realistic picture of typical variation. Plotting +/- 2 standard deviations stretches the comparison to the absolute limits of the population. When coupled with artificially shrunken confidence intervals, this practice inflates the visual distinction between group trajectories.

Although the statistical inferences in the text rely on conventional 95% significance thresholds, the figures must accurately reflect that same standard of evidence. If the 95% confidence bands

overlap at +/- 1 standard deviation, that overlap is an accurate reflection of model uncertainty and must be presented transparently. Furthermore, the reluctance to increase the transparency of the shading is perplexing. Adjusting the alpha level of the ribbons is a standard and simple solution to improve visual presentation and handle overlapping error bands without compromising statistical integrity.

Therefore, I must insist that the figures be revised to display standard 95% confidence intervals at +/- 1 standard deviation, thereby aligning the visual presentation of uncertainty with the reported statistical tests. Transparency adjustments should be utilized to ensure trend lines remain visible where confidence bands overlap. I also urge the authors to use distinct colors or tones, as the current contrasts are too similar and will be difficult for colorblind readers to distinguish (a challenge I face personally as a visually impaired individual).

In the previous round, I noted: “Sixth, while the sample size appears adequate, presenting a power sensitivity analysis would be beneficial. I understand that the tested model is complex, but providing an approximation would be helpful, especially since the class level seems to have a relatively low number of units.” While the authors presented an approximation in response (referring to norms), it is necessary to explicitly state the minimum effect size the study was adequately powered to detect. I believe MPLUS has power analysis related functions to identify these.

I appreciate the expansion of the introduction and abstract to capture the striking gender differences. I still strongly recommend providing a graph to visualize these disparities. Readers need to know whether the associations are strictly observed in one gender or merely attenuated among women and a graph would solve this while ensuring high accessibility for readers.

Additional Issues

Regarding political dissatisfaction, the manuscript states that constraining paths led to better fit. However, this improvement is not obvious, and no direct chi-square difference test is reported (such comparisons should be provided for all nested models). For example, on certain indicators, the model fit appeared to worsen, shifting from χ^2 (21) = 47.31, p = .001, RMSEA = .038, SRMR = .038 to χ^2 (34) = 62.93, p = .002, RMSEA = .031, SRMR = .045.

At one point, the text states, “The internal reliabilities were good .71, .75, .78, .79, and .81 for T1-T5, respectively.” Given that several of these values are more accurately described as acceptable rather than good, please adjust this wording.

In the participants section, the manuscript notes that the sample is “from diverse socioeconomic (M = 3.01, SD = 0.80)” backgrounds. This statistic is uninterpretable prior to reading the instruments

section. Additionally, because the survey includes three socioeconomic status questions, it remains unclear which specific variable this estimate references.

The text reports significant differences between groups (e.g., lines 303–306) but subsequently reports mean differences where the 95% confidence intervals include zero, such as “0.10 (95% CI [-0.00, 0.20])”. This discrepancy is confusing. Furthermore, when a rounded two-decimal estimate results in zero, please include an additional decimal place (or consistently use three decimal places for confidence intervals throughout the manuscript, as this currently varies). Finally, please ensure that eta-squared effect sizes are reported consistently; currently, their inclusion varies throughout this section.

Thank you for the opportunity to re-evaluate this important work. I appreciate the authors' responsiveness and hope they are receptive to implementing these remaining and additional requests, which should help bring the manuscript close to publication.

Round 3

Editor

Date: 2026-06-15

Dear Authors,

Thank you for the careful and thorough work you have put into this revision. You have addressed the great majority of the points from the previous round, and the manuscript has improved considerably as a result. The reporting is now cleaner and more consistent, the reference list is in much better shape, and moving the RI-CLPM result summaries along with the robustness and measurement analyses into the main text has made the contribution noticeably easier to follow. We are very close to acceptance, and only a few issues remain before we can finalize a decision.

1. Thank you for summarizing the RI-CLPMs in the main text. I understand that you are unable to produce the figure. As a compromise, please also move the statistical results for it to the main text. This change does not require further analysis.

2. In the abstract, please describe the reciprocal finding explicitly. Specifically, change “There were no significant lagged effects between radicalism and alienation, except for dissatisfaction.” to “For political dissatisfaction, reciprocal relationships were observed with radicalisation in a cross-lagged panel model.” As outlined in the previous decision letter, this is a substantial finding and should be communicated clearly.

3. In the results, it is enough to present the Hamaker reference once. Relatedly, the within-person descriptions read somewhat oddly when repeated in linguistically identical form across sections, as the wording was clearly carried over. I would suggest rephrasing these passages so they no longer read as copied.

5. Please also adjust the following sentence in the abstract: “The findings suggest that adolescents’ experiences of political distrust, powerlessness, and dissatisfaction with the functioning of the political system increase the likelihood of radical political behavior.” The word “increase” overstates the causal claim, given the results. I would suggest something closer to: “The findings suggest that adolescents’ experiences of political distrust, powerlessness, and dissatisfaction with the functioning of the political system are associated with the likelihood of radicalism across developmentally critical periods.”

6. The references have greatly improved, thank you. One inconsistency remains: “Pfundmair, M., Paulus, M., & Wagner, E. (2021).” is cited as 2020 in one instance. Please reconcile the year.

7. To improve readability, please change the alpha of the 95% CI ribbon in all figures.

8. Please provide figures in SVG format. This is required per the journal’s standard.

I look forward to receiving the revised manuscript, which I anticipate will be the final round.

Sincerely,

Milan Obaidi

Round 4

Editor

Date: 2026-07-08

Dear Authors,

Thank you for addressing our remaining concerns. I am happy to accept the paper for publications as is. It addresses an important gap in the literature and I expect it to be of significant impact to the field.

Sincerely,

Milan Obaidi